dependent variable involves the logarithm of the share) to be somewhat misleading, as it seems to confuse a zero share with the separate issue of missing data.

Chapter 6 introduces the "competitive map" and illustrates it with the coffee example. The map is an attempt to aid interpretation of the large matrix of own- and cross-price (and other marketing action) elasticities by plotting brands in a lower dimensional space defined by eigenvectors of the estimated elasticity matrix. Two vectors are plotted for each brand, corresponding to the brand's "clout" (its influence on other brands' market shares) and its "receptivity" (the effect of other brands' actions on its share); the extent to which brand *i* is influenced by brand *j* is proportional to the cosine of the angle between *i*'s receptivity and *j*'s clout vector. Use of these maps appears to have promise, but the subtleties in their construction and the apparent inability to interpret the dimensions of the map pose difficulties. The authors seem to acknowledge as much by their disclaimer: "The full meaning of [the signals offered by a map] is better interpreted by managers and management scientists involved in these markets than by academic researchers involved in methods development" (p. 201).

Chapter 7 describes a market information system oriented to market-share analysis, developed by one of the authors. The system appears to be most useful as a training device, for classroom or corporate use. It provides graphical devices for analyzing market experience and the capability to simulate the results of a variety of market strategies.

The concluding chapter offers a fairly short list of topics the authors suggest are most deserving of future research. These range from estimation of the current models to integration of game theory and use of POS data from consumer panels. In my view, issues of the reliability of estimated elasticities and the stability of competitive maps are also important. Although not addressed in the book, the reliance on observational data to estimate tens or perhaps hundreds of elasticities is problematic, as each brand's mix of marketing variables is determined endogenously. Further work is required to determine how reliably simulations embedding these estimates can forecast the effects of new strategies, and thus how useful the estimated elasticities are as guides for marketing decisions.

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Charles Brown, James Hamilton, and James Medoff, *Employers Large and Small.* Cambridge, Mass.: Harvard University Press, 1990. Pp. 144. ISBN 0-67425-162-8.

The basic argument of this 91 page presentation is that small firms, compared to large firms: (1) provide [only] a modest share of new jobs; (2) have lower wages; (3) poorer fringe benefits; (4) confront a higher per-employee cost for their labor force; (5) have poorer working conditions; (6) provide less job security; (7) are owned by wealthy individuals; and (8) have considerable political influence, particularly with regard to decisions undertaken by the Federal government. They conclude, therefore, that the popular image of small businesses as a major benefactor to the economy — despite a lack of political influence — is inaccurate. The authors insist that they have shown the converse to the true — that small businesses have substantial, political influence despite only modest, insignificant contributions to the economy. They propose interventions designed to benefit workers, which would make working conditions in small businesses equivalent to the conditions in large businesses. They do not attempt to demonstrate that the working conditions in typical small businesses are not socially acceptable. In fact, they only focus on comparisons of small and large businesses.

A substantial amount of research is reviewed in support of the two main features of their argument: (1) the advantages for working with a large firm; and (2) the disproportionate political influence of small firms.[1] One major implication of their presentation is that, since it is more advantageous to work for large firms, which provide about half of all jobs, small firms should have less political influence than they do. This is a curious position to advance.

In the U.S. it is currently assumed that influence on the political process is (and should be) related to the capacity to mobilize voters — individually or collectively — to pressure elected officials. This influence may take the form of personal contact, letters, or political contributions. The authors apparently find the traditional mechanisms for affecting the political process to be unsatisfactory. They do not, however, provide any alternative, only a complaint that one group of voters, casually defined as "small business owners," have more influence than is justified by their economic contribution to society.

Their argument implies that a diminished presence of small businesses would substantially benefit the U.S. economy. The authors' analysis suggests that, if the political influence of small businesses were somehow reduced, the nations' workers would be better off. First, small businesses would lose their exemption from much Federal regulation in the workplace, thereby enchancing the work environment for small business employees. Second, without the favored treatment for small businesses, large business employment would expand quickly, and the displaced workers would be better off, with new jobs, improved pay, liberal benefits and, of course, job security.

The authors make three assumptions that are worth debating: first, that the socio-economic-political system is a zero sum game. That is, if political action were to favor small business (either through subsidies or the relaxation of regulatory standards), then there must be a loser, and large businesses and its employees are those losers. Secondly, that small and large businesses are separate, alternative systems of production. Under this assumption a shift in production from small to large is quite straightforward, with the major winners being workers, the losers the small business owners. Third, and much more subtle, that modern, advanced economies, such as the U.S., are static entities. That the technological and organizational systems that produce goods and services are relatively fixed and that only the actors (producers and consumers) change over time. The changes in organizational entities — births, deaths, expansions, contractions — reflect nothing more than changing fortunes in a static, competitive arena. One restaurant drives out another; one builder attracts the clients of another, etc. Hence, changing the rules of the game will have little impact on the ultimate benefits provided by the economic system (GNP, jobs, new products and services, etc.).

From the perspective of improving the country's competitive position, the authors' analysis has ignored one central issue: the dynamic interdependences between different sized firms and the increasing turbulence among all business.

The dependence of small firms on large firms is widely accepted. Every state — every community — has examples of a major corporation drawing supplies, parts, subassemblies and services from a host of small firms. Descriptions of the extent to which a wide range of small firms are dependent upon a single, large firm are a prevalent feature of any large firm request for public subsidy and other assistance. The example of the suppliers to Chrysler assisting in lobbying for Federal loan guarantees to Chrysler is eagerly reviewed in this book (p. 74).

Less widely recognized, however, is the extent large firms are dependent upon small firms. This is dramatically illustrated by the location of the new automobile assembly plants in the United States. Whether they be establishments of domestic or foreign corporations, they have all located where there are high concentrations of small and medium firms that supply critical parts and components. These assembly plants are heavily dependent on the ambient population of small firms. The geographic mobility of most large firms, or their branches, is substantially constrained by the need for ready access to small firms which provide the necessary goods and services.

Wilbur Maki and I have completed a research study for the U.S. Small Business Administration, analyzing the impact changes in small and large frim branches have had on economic growth in 382 U.S. labor market areas.[2] In general, our analysis suggested that labour market areas with higher turbulence (additions and losses) in both establishments and jobs had greater subsequent economic growth. Higher rates of business births and deaths, as well as jobs created and destroyed, were followed over the next two years by a greater net growth in total jobs, labor income and property income.

The high level of turbulence is seldom recognized in the U.S. economy. For the past several decades, about one in five business entities (independent firms or branches) have been either founded or discontinued annually. By 1986, one in four business jobs was either created (through business births or expansions) or destroyed (through business contractions or discontinuances) *annually*. This is up from an annual rate of one in eight in 1976. Over the same period, 1975—86, over 21 million additional business jobs were created; half by small firms — half by large. Compared to the substantial net job growth, the relative importance of small versus large firms seems to be a trivial concern. Still, it is all that our authors seem to be concerned with.

Business turbulence reflects a variety of underlying processes. A significant part is normal turnover when owners discontinue businesses to retire or shift to new endeavors. But turbulence also reflects shifts in productive technology, as firms with newer, more efficient processes replace those with older, less efficient processes. It also reflects change in the demand patterns of other, upstream businesses and ultimately consumers, as their needs and tastes change. Some of the shifts are accounted for by a redirection of specialties within existing firms; other shifts are accounted for by the birth and death of independent businesses.

A focus on the relative political influence of small versus large firms implies that the businesses that make up the U.S. economy are a static, unchanging set of players competing for a given quantum of political influence. A zero-sum game as it were. It detracts for the major issues that currently confront the U.S., maintaining a competitive system that will ensure economic well-being which is a necessary condition for a humane and just society. A focus on issues such as turbulence in business entities, job volatility and the interdependence between businesses of different sizes — is what is curiously absent from the book under review.

While it is appropriate that workers (the constituency that the authors claim to represent) are entitled to a minimum level of wages, fringe benefits and safety equipment, no evidence is presented to suggest that this is not the case for typical, small business employees. All citizens, workers, owners and others — are entitled to the benefits of a productive, competitive economy. All firms, large and small, have a unique role to play in modern economic systems. What is sorely needed is not a political commentary on large versus small firms, but an enhanced understanding of how the dynamic interdependence of small and large firms contributes to an adaptive, competitive economy. The book under review fails to provide this perspective.

Notes

[1] The details of their analysis and inferences are not above criticism, but they cannot be covered in this review.

[2] Reynolds, Paul D. and Wilbur R. Maki. 1990. *Business Volatility and Economic Growth*. Project report completed for the U.S. Small Business Administration, Contract SBA 3067-OA-88.

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